This paper considers variability in *in situ* ^{10}Be concentration in one catchment draining most of the central Himalayas in order to understand how temporal variability in ^{10}Be concentration may affect estimated erosion rates. Overall I think the paper is an interesting contribution to add to empirical work previously done by Lupker and Gonzalez and modeling done by Niemi and Yanites. I have a few concerns about the paper that I think the authors could address fairly easily to make the paper acceptable for publication.

1) The authors have completely ignored the Earth Surface Processes and Landforms paper by Gonzalez that came out last year. In that paper Gonzalez and coauthors (full disclosure that I am the corresponding author on that paper) compare ^{10}Be (both *in situ* and meteoric) measurements of pairs of samples from western China and also do a meta-analysis of previously published replicate studies. Not including this paper is a major oversight because it means that it feels like Dingle is reinventing the wheel rather than engaging in conversation with other manuscripts considering similar topics. In particular, I think that considering similarities and differences to the Gonzalez et al dataset would be appropriate for the discussion and the legwork Gonzalez did in a global meta-analysis should contribute to the introduction.

2) I am bothered by the way that the manuscript is structured with respect to the landslide modeling. The manuscript has a standard set up of introduction, methods, results, discussion, conclusion for the ^{10}Be concentrations and derived denudation rates. However, the authors have thrown into the discussion section all of the background (introduction), methods, results, and discussion for their landslide model. I think that it would be more appropriate to introduce the model early, develop it in the methods, present the results in the results section, and discuss it in the discussion section.

3) Although the authors do an excellent job of addressing a number of possible reasons for the variability in isotopic data, they do not (as far as I can tell) mention the one process that Lupker concluded dominated in the Himalayas – varying sourcing from catchments with similar average erosion rates but different production rates due to varying elevation.

4) I'm confused about why you use concentration differences when you have location differences for your samples. It seems like in that case it is better to compare denudation rates (as Schaller did) rather than comparing concentrations, which are biased by variable upstream elevation. I agree that if you are comparing two samples at the exact sample (or nearly exact same) sample site, then comparing concentration is more appropriate, but given the variability in location, I think that comparing denudation may be better.

5) I would like to see some conclusions that you can draw that may help others to sample better. It's all well and good to say "be careful, the system doesn't work like you assume" but unless you can offer concrete suggestions, people will continue to happily run around and grab bags of sand and assume that everything is well averaged. What can you recommend we do?

In addition to these major points, I have a few minor comments:

Term use throughout: Why do you use CRN instead of ^{10}Be? You only measured *in situ* ^{10}Be, so it seems silly to refer to the generic term CRN throughout.

Acronyms throughout: Be sure to define all acronyms (CRN, CAIRN, CRONUS) at first appearance.

P5 L26: I would put the grain size you field sieved to here.

P6 L7: Can you justify that sometimes including this finer grain size doesn't affect your concentration measurements?

Section 3.2: I think it would be extremely helpful to have a table that has the quartz amount, carrier mass, and AMS measurements (ratios, blanks, blank-corrected ratios) so that calculations can be checked and replicated. These are standard supporting tables these days. This is especially important since your blank values range from 4 to 54% of your measured ratios.

Section 3.3: Be sure to include all the parameters that one needs to replicate your work (like assumed sediment density). It is also useful to include a table that one can easily input into an erosion rate calculator should they want to do a meta-analysis or recalculate your denudation rates as parameters change.

Figures: All figures have text that is hard to read. I assume it is a reproduction for review issue, but wanted to ensure that you check that text is easily legible in final publication format.

Figure 2: If you aren't using lat/long, you need to say what datum and projection your coordinates are from. Given that Table 1 has lat/long in decimal degrees, you may consider changing figure 2 to use decimal degrees as well.

Table 2: Why mean elevation instead of effective elevation? (And, I am assuming "average" is "mean" and not "median" or "mode". Probably would be good to clarify.)

This review is intended to be constructive and I would be happy to clarify or answer questions for the authors.

Amanda Schmidt